Reliable knowledge

An exploration of the grounds for belief in science

JOHN ZIMAN, FRS

Emeritus Professor of Physics, University of Bristol

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'Science repudiates philosophy. In other words, it has never cared to justify its truth or explain its meaning.'

Alfred North Whitehead

1.1 *The challenge*

This work arises from two sources: a *challenge* and a *theory*. The challenge is to the beneficience of science as an agent of social change: the theory concerns the nature of scientific knowledge.

The attack on science comes from many quarters, but is not well concerted. The medley of opposition includes many strange companions-in-arms, following contradictory causes. The conservative fears that science will destroy the only world that he knows; the progressive imagines that it will poison the paradise to come; the democrat is cautious of the tyrannous capabilities of technique; the aristocrat fears the levelling tendency of the machine. The pleas of defence are equally inconsistent: some say that scientific progress is automatic and inevitable; others that the future must be determined by rational scientific planning; technocrats delight in telling us that science will make life more comfortable; space addicts proclaim that man must go forth and conquer the universe.

Science is such a complex human activity, so much a part of our civilization, so rapidly changing in form and content, that it cannot be judged in a few simple sentences.[1] We observe, nevertheless, that some of the products of scientific technology have been damaging to human welfare. In such cases, one can usually blame factors outside the realm of science: too hasty innovation, subordination to unworthy causes, distortion of social needs, or displacement of genuine human goals. But the feeling has arisen that the evil factor is knowledge itself; science is characterized as a materialistic, antihuman force, a Frankenstein monster out of control.

More subtle critics[2] do not minimise the instrumental power of

[1] This statement needs no documentation. *The Force of Knowledge* (1976: Cambridge University Press) is my personal view of the sociological and historical background to the present work.

[2] Exemplified admirably by Theodore Roszak (1972) in *Where the Wasteland Ends* (London: Faber).

science in its material, technical mode. The reliability of scientific knowledge in engineering, manufacturing, or medicine is not really in doubt. But they resist the attempt to extend science to the niceties of biological behaviour, human emotion and social organization. Any claim to scientific authority in such matters is regarded by such critics as pretentious, and inherently unsound. Other sources of insight and other guides to action must be treasured or sought beyond the reach of the scientific method.

This is the challenge – and it must be treated very seriously. A century ago, we might have described it as the conflict between Science and Religion. Nowadays, most people no longer base morality and aesthetics on divine revelation or rational theology; but no mature person with experience of life can seriously suppose that the issues of love and death, of justice and charity, could possibly be resolved by consulting the *Handbuch der Physik* or some latter-day edition of an *Encyclopaedia of the Behavioural Sciences.* On such matters, science clearly has little to say.

On the other hand it prejudges the issue to presume that a 'method' that has proved its worth in the realm of material technique can tell us nothing of value concerning man in society. We humans are part of the natural order of things, and subject to its necessities. The response to the challenge can be neither outright defiance nor abject surrender; the field of conflict is the middle ground, where the claims of science can be seen to be neither fanciful nor beyond reasonable doubt.

For this reason, the question of the *reliability* of scientific knowledge has become a serious intellectual issue. Once we have cast off the naive doctrine that all *science* is necessarily *true* and that all *true* knowledge is necessarily *scientific*, we realize that *epistemology* – the theory of 'the grounds of knowledge' – is not just an academic philosophical discipline. Very practically, in matters of life and death, our grounds for decision and action may eventually depend on understanding what science can tell us, and how far it is to be believed.

1.2 *The theory*

But what *is* science? How is it to be distinguished from other bodies of organized, rational discourse, such as religion, politics, law, or 'the humanities'? In an earlier work,[3] I have tried to show that scientific

[3] *Public Knowledge* (1967: Cambridge University Press).

knowledge is the product of a collective human enterprise to which scientists make individual contributions which are purified and extended by mutual criticism and intellectual cooperation. According to this theory *the goal of science is a consensus of rational opinion over the widest possible field.*

From this point of view, much can be understood about the ways that scientists are educated, choose research topics, communicate with one another, criticize and refine their findings, and relate to one another as members of a specialized social group. The 'consensus principle' thus leads directly into what is now called the *internal sociology* of the scientific community. From there we naturally proceed to investigate the place of science in society at large, trying to throw light on such important practical questions as the economics of research and development, the organisation of scientific institutions, priorities and planning of research, and the agonising ethical dilemmas facing the socially responsible scientist.

It is undoubtedly of great value to understand *how* science is done, and to appreciate the social role of the scientist and his institutions. But the epistemological challenge strikes deeper. What are the characteristic features of the body of knowledge acquired by this means? How does the consensus principle determine the *content* of science? What sorts of statement, about what aspects of the totality of things, are legitimate candidates for validation as 'public knowledge'? And to what extent does the striving for consensus eventually provide adequate grounds for belief and action?

In this book, therefore, I have deliberately turned away from the sociological aspects of science, to reconsider these fundamental intellectual issues. I am fully aware, of course, of the immense literature on the philosophy of science, where these very questions are asked again and again, and given a whole rainbow of answers. The writings of Plato and Aristotle, Bacon and Descartes, Kant and Wittgenstein, on this subject are the common heritage of our culture. But not being a trained philosopher, I could not pretend to be acquainted with all past and present opinions, all insights and all objections, on so large a topic.

Instead of attempting a general assessment of the epistemological problem, I propose to adopt the intellectual strategy of a typical paper in theoretical physics. A model is set up, its theoretical properties are deduced, and experimental phenomena are thereby explained, without detailed reference to, or criticism of, alternative hypotheses. Serious objections must be fairly answered; but the aim is to demon-

strate the potentialities of the theory, positively and creatively, 'as if it were true'.

The argument developed in the following pages is not, therefore, deeply embedded in the conventional philosophical literature. Ideas have been drawn from a variety of fields, such as linguistics, computer programming and anthropology, where I have had to sample and browse unsystematically on the look out for information or inspiration. In many particulars, however, the opinions expressed are far from novel, and are already to be found in the writings of some well-known philosophers.[4] I have done my best to cite such authorities – not only to do them justice, but also to bolster my own case. But I have not attempted to comb the literature for every hint of the same point of view – or for every possible objection to it; ignorance or neglect of work that might seem relevant to each particular issue is regrettable, but will, I trust, be forgiven.

1.3 *The model*

To characterize science fully, we must describe it in all its aspects – sociological, psychological and philosophical. For the purposes of this book, however, we need only consider a simplified model where the sociological dimension is reduced to a very simple 'Mertonian' scheme.[5] The relations between individual scientists (or between groups such as 'research teams') are assumed to approximate reasonably well to the Mertonian norms; in other words, they behave honestly to one another, both in communicating their own work and in accepting the work of others.

This idealization is essential if the epistemological issue is not to be hopelessly confused. We know, of course, that no scientific community is entirely healthy in this respect, and that the credibility of science as a whole is occasionally damaged severely by pathological deviations from the norms (6.5). Imperfections of communication or of critical analysis reduce the reliability of science in every field. In practice, however, this is seldom the dominant factor affecting credibility; the fragmentation and sectarianism characteristic of some fields of research (e.g. psychiatry) are not so much symptoms of social breakdown as consequences of the immense difficulty of making any progress at all in understanding the subject.

[4] For example, N. Capaldi (1975) in *Determinants and Controls of Scientific Development* edited by K. D. Knorr, H. Strasser and H. G. Zilian (Dordrecht: Reidel) presents a very schematic outline of the general point of view adopted in this book.

[5] See Robert K. Merton (1973) in *The Sociology of Science* (University of Chicago Press) pp. 267–78.

Although scientists often promise immeasurable future delights of understanding and truth, the epistemological challenge is always uttered at a particular moment: 'What can we believe *now*?' In assessing the credibility of scientific knowledge, we naturally look back over the past, but can put little weight on prognostications of an uncertain future. Our model, therefore, must be historically accurate, but need not be self-propelling; it will seldom be necessary to refer in detail to the psycho-dynamic forces that continually transform the contents of science (6.7).[6]

This is fortunate, since discussion of intellectual 'creativity' always tends towards a logical impasse – to cerebrate by other means than those of a particular science the unknown concepts that will eventually arise in that science. We shall see, indeed, that much more down-to-earth intellectual phenomena of belief and doubt, where the subject matter and context are known in advance to the psychological investigator, are also connected with 'creative' powers of imagination and intuition (5.4).

On the other hand, we cannot adopt a 'freeze-dried' model, where, on the appointed date, dispassionate, unprejudiced recording angels fly down to examine the scientific archives, and make an absolute assessment of the validity of each scrap of knowledge. As we saw in § 1.1, the epistemological challenge is not just an academic question; it arises in a human situation, and the answer is often required to deal with a human predicament. Those who ask the question, 'is this a matter on which science is to be believed?' must be given an answer that takes into account their own biographical experience and capabilities of comprehension. It would have been misleading, for example, to tell a railway engineer in 1920 that he should no longer believe in Newtonian mechanics because it had just been superseded by Einstein's general theory of relativity; for all his purposes, Newton's laws of motion remain as true as ever. From the very beginning I reject any system of *metascience* that purports to have such angels at its beck and call.[7]

[6] The numbers in parentheses are cross-references to other sections.

[7] This applies, in particular, to 'logical empiricism', in the various forms criticized by G. Radnitzky (1968) in *Anglo-Saxon Schools of Metascience* (Goteborg: Akademiforlaget). But I also, most emphatically, reject his hubristic view (p. xiv) that 'the metascientist will, one day, function like the business *consultant* – he will have to advise, warn, etc. in connection with the knowledge-producing enterprise, be it for the purpose of the planned production of some specific piece of knowledge or know-how, or be it for the regulation of the available "scientific capital" of a nation, a firm, etc. by means of foreign trade in scientific knowledge'.

1.4 *Consensibility and consensuality*

In its simplest form, therefore, our model consists of a number of independent scientists, linked by various means of *communication.* Each scientist makes observations, performs experiments, proposes hypotheses, carries out calculations, etc., whose results he communicates to his colleagues. As an individual, the scientist, like any other conscious being, acquires a great deal of personal knowledge about the world he inhabits, not only through his own experience but also through the information flowing to him from others. But when we talk of scientific knowledge, we refer to the content of the messages that accumulate and are available in the public domain, rather than to the memories and thoughts of each person.[8]

Going beyond this truism, we shall assume that scientific knowledge is distinguished from other intellectual artefacts of human society by the fact that its contents are *consensible.* By this I mean that each message should not be so obscure or ambiguous that the recipient is unable either to give it whole-hearted assent or to offer well-founded objections. The goal of science, moreover, is to achieve the maximum degree of *consensuality.* Ideally the general body of scientific knowledge should consist of facts and principles that are firmly established and accepted without serious doubt, by an overwhelming majority of competent, well-informed scientists. As we shall see, it is convenient to distinguish between a *consensible* message with the *potentiality* for eventually contributing to a consensus, and a *consensual* statement that has been fully tested and is universally agreed. We may say, indeed, that consensibility is a necessary condition for any scientific communication, whereas only a small proportion of the whole body of science is undeniably consensual at a given moment.

This model imposes constraints upon the *contents* of science. In the first place, fully consensible communication requires an unambiguous *language,* of which the ideal form is *mathematics* (2.2). But the exchange of logically consistent messages is fruitless unless they refer to recognizable and reproducible events within the experience of individual scientists; this explains the fundamental role of controlled observation and *experiment* (3.3) in the conventional 'method' of science.

But human cognition and communication are not restricted to pointer readings and algebraic formulae. Through our natural facility

[8] This is evidently 'World 3' of Karl Popper's *Objective Knowledge* (1972: Oxford University Press) – the logical contents of books, libraries, computer memories, etc. (5.5).

for *pattern recognition* (3.2) we may become aware of significant features of our experience, and transfer consensible messages, in the form of diagrams and pictures, whose 'meaning' cannot be deduced by formal mathematical or logical manipulation. For this reason, scientific knowledge is not so much 'objective' as 'intersubjective' (5.6), and can only be validated and translated into action by the intervention of human minds. In this respect, our model is less restrictive of the legitimate contents of science but offers less hope of strict testing of reliability than many conventional epistemological schemes.

These messages are not merely poured out into the archives nor passively received by other scientists. Consensuality implies strong interactions between the human actors in the drama. Thus, for example, elementary errors and misunderstandings are eliminated by the independent repetition of experiments, or by theoretical criticism. The fact that every competent scientist is trained – or moulded by bitter experience (3.4) – to the highest levels of self-critical precision in his communications (6.3) does not mean that this aspect of the model can be ignored; trivial errors are endemic in scientific research and must be continually corrected if the system is to generate anything approaching 'the truth'.

In the effort to maximize the area of consensus, however, the scientific community goes far beyond the exchange of easily corrected factual communications. Theoretical systems that explain the actual facts and imply a multitude of other potentially observable results are postulated. The consensuality of such systems is tested by such strategies as the attempted confirmation of predictions (2.8) or by the discovery of marginal phenomena that might prove inconsistent with accepted theories (3.6). It is important to realize that much of the research literature of science is intended *rhetorically* – to persuade other scientists of the validity of a new hypothesis or to shatter received opinions.

Looking now at our model as a whole, we easily recognize the power that participation in such activities has over the minds of individual scientists. Beyond the limits of their own personal observations of nature, they are aware of the immense body of results obtained by their predecessors and contemporaries, under stringent conditions of mutual criticism and reinforced by the persuasive authority of striking discoveries and astonishingly successful predictions. The relatively coherent and consistent set of beliefs thus generated is what we call a scientific *paradigm* or 'world picture' (4.4).

Nevertheless, despite its coherence and consensuality, such a para-

digm is not necessarily close to 'absolute truth'. As has been emphasized, our model of science does not contain any independent source of 'objective' knowledge, and is therefore vulnerable to error in two significant ways.

In the first place almost every scientist is raised up, by formal education and research experience (6.2), within the world picture of his day, and cannot happily consent to statements that are obviously at variance with what he has learnt and come to love. The achievement of intersubjective agreement is seldom logically rigorous; there is a natural psychological tendency for each individual to go along with the crowd, and to cling to a previously successful paradigm in the face of contrary evidence. Scientific knowledge thus contains many fallacies (4.5) – mistaken beliefs that are held and maintained collectively, and which can only be dislodged by strongly persuasive events, such as unexpected discoveries or completely falsified predictions. In other words, our model must take into account the effects of its collective intellectual products on the cognitive powers of each of its individual members.

Secondly, and more significantly, is there any defence against the charge that the whole scientific paradigm is a self-sustained delusion (5.10)? The scientists in our model are almost always deliberately trained to a particular attitude to natural phenomena. How are their intellectual constructs to be distinguished from those of any other self-accrediting social group, such as a religious sect? What reason have we for preferring the scientific paradigm as the ideal, unique world picture?

We may assert that the social system of science is always open to the outsider (6.3), and that contributions of fact or opinion are not solely restricted to registered True Believers. It is well known, for example, that major scientific progress often comes from scientists who have crossed conventional disciplinary boundaries, and have no more authority than a layman in an unfamiliar field. According to the ethics of 'the scientific attitude', science is valid in principle for Everyman, because *any* man could, if he wished, take up the study of science for himself, and would eventually be freely persuaded of its truth.

In practice, however, this is almost impossible; and when we look at the brainwashing implicit in the long process of becoming technically expert in any given branch of science, we see that it scarcely answers the objection – he who emerges from this process is no longer the unbiassed independent inspector who entered it ten years before.

More to the point, it must be emphasized that no scientist is a

disembodied observing and conceptualizing instrument; he is a conscious human being, born and reared in the common life of his era. Long before he is taught about electrons, and genes, and exogamous fratries, he has acquired practical experience of pots and pans, cats and dogs, uncles and aunts. Although such mundane objects are seldom discussed as such in high science, they are not excluded from its realm. However fantastic it may appear on its wilder shores, the scientific consensus includes, by definition, the matter-of-fact, and must be coherent with everyday reality (5.10). Failure to accord with reliable 'commonsense' evidence is quite as discreditable as falsification of a theory by a contrived, abstruse experiment. Of course, commonsense evidence may often turn out to be irrelevant or ambiguous, but it cannot be trampled underfoot.[9]

The epistemological challenge to science thus leads to such profound questions as how each person acquires his view of the world, how far all men see the same world, and whether there can be any conceivable alternative to the 'reality' in which most men believe. The answers to these questions must not be anticipated here, for they determine the whole outcome of this book.

In some respects, however, this outcome cannot really be in doubt. Science does, after all, have its triumphs. It would be absurd to deny the validity of a theoretical system such as quantum mechanics, to which we owe our stocks of nuclear weapons. Who would doubt the credibility of Mendelian genetics, now completely confirmed at the molecular level by the deciphering of the genetic code? At least *some* of the knowledge that has been acquired 'scientifically' is as reliable as it could possibly be.

The basic strategy of this book is, therefore, to illustrate the workings of the social model of science by reference, initially, to the 'natural sciences', where the power of the 'scientific method' has been demonstrated beyond reasonable doubt. The most astonishing achievements of science, intellectually and practically, have been in *physics*, which many people take to be the ideal type of scientific knowledge. In fact, physics is a very special type of science, in which the subject matter is deliberately chosen so as to be amenable to quantitative analysis (2.7). But it is only when we have fully understood how

[9] In other words I accept the viewpoint summarized by G. Santayana (1962) in *Reason in Science* (New York: Collier Books) 'Science. . . is common knowledge extended and refined. Its validity is of the same order as that of ordinary perception, memory, and understanding. Its test is found like theirs, in actual imitation, which sometimes consists in perception and sometimes in intent. The flight of science is merely longer from perception to perception, and its deduction more accurate from meaning to meaning and from purpose to purpose.'

science really works even under the most favourable conditions that we can appreciate its limitations. For that reason, I felt it necessary to discuss the 'philosophy' of physics at some length, especially in Chapters 2 and 3. Of course, this is difficult, because physics is a very sophisticated intellectual discipline, whose techniques and attitudes are not easily explained to the uninitiated; I hope that I have managed at least to hint at some of this, by reference to various historical and contemporary examples, without losing the reader on the way. No doubt quite similar case histories could be found in chemistry, geology, or biology, but they would not necessarily be any easier to grasp out of context.

This investigation of the epistemology of the natural sciences takes up the greater part of the book. It is only in the final chapter that we arrive at a position from which we can begin to consider the fundamental question of the book as a whole – how much ought we to believe of what science might tell us about man as a conscious social being, subject to unreasonable emotions and irrational institutions? I do not pretend that such a question *can* be 'answered', but it seems appropriate to subject it to a scrutiny based upon all that we have learnt about the credibility of the natural sciences, where the subject matter is so much easier to control. The results of this scrutiny are not, to tell the truth, very favourable to the 'behavioural sciences' as we know them today; perhaps, after all, the epistemological challenge is not unjustified in that respect.

Needless to say, this inquiry is entirely concerned with the *cognitive* aspects of science and not at all with any instrumental applications of scientific knowledge to technology or other human activities. A successful application of knowledge is, of course, a pragmatic demonstration of its validity, and much of what is referred to as 'observation' or 'experiment' in fact derives from carefully recorded practice. Similarly, a confirmed or falsified prediction may have been derived from a very practical event, such as the failure of a carefully designed bridge. The main themes of this book may seem academic and aloof; but in a society dazzled by silver-tongued technocrats and other self-accrediting experts these questions are only a few breaths away from harsh realities and bitter home truths.